

SUMMARY OF THE WORKSHOP
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1 Overview

The title of the workshop, “The QCD Phase Transitions”, in fact happened to be too narrow for its real contents. It would be more accurate to say that it was devoted to different phases of QCD and QCD-related gauge theories, with strong emphasis on discussion of the underlying non-perturbative mechanisms which manifest themselves as all those phases.

Before we go to specifics, let us emphasize one important aspect of the present status of non-perturbative Quantum Field Theory in general. It remains true that its studies do not get attention proportional to the intellectual challenge they deserve, and that the theorists working on it remain very fragmented. The efforts to create Theory of Everything including Quantum Gravity have attracted the lion share of attention and young talent. Nevertheless, in the last few years there was also a tremendous progress and even some shift of attention toward emphasis on the unity of non-perturbative phenomena. For example, we have seen some efforts to connect the lessons from recent progress in Supersymmetric theories with that in QCD, as derived from phenomenology and lattice. Another example is Maldacena conjecture

and related development, which connect three things together, string theory, super-gravity and the (N=4) supersymmetric gauge theory. Although the progress mentioned is remarkable by itself, if we would listen to each other more we may have chance to strengthen the field and reach better understanding of the spectacular non-perturbative physics.

That is why the workshop was an attempt to bring together people which normally belong to different communities and even cultures (they use different tools, from lattice simulations to models to exact solutions), in order to discuss common physics. It was a very successful, eye-opening meeting for many participants, as some of them said in the last round of discussions. Even organizers (who of course have contacted many speakers in advance) were amazed by completely unexpected things which were popping out of one talk after another. Extensive afternoon discussion, in which we always return back to the morning talks, has helped to clarify many issues.

For QCD one of the main source of “input” remains experimental data about hadrons. The second, now nearly as important as the first, is provided by numerical lattice simulations. Those can also consider various flavor contents, change the quark masses, easily access finite temperatures (finite density remains so far a problem). Furthermore, they can study observables not in average, but on configuration-by-configuration basis, and reveal more details about a dynamics. The third major input is provided by exactly solvable (or partially solvable) models, mostly the Super-symmetric (SUSY) ones.

Let me on the onset indicate some similarity between various approaches discussed on the workshop. Many (if not most) of the talks in this way or another separate “quantum noise” (the perturbative phenomena) from “smooth” or even classical fields, related to non-perturbative dynamics. The tools used for this general aim are however very different: (i) Blocking lattice configurations, or “cooling” them; (ii) Considering super-symmetric theories in which many diagrams cancel; (iii) Considering large N_c limit, in which there should be some “master field” dominating the path integrals (Mattis again); (iv) going to complex-valued configurations, which are some non-trivial saddle points (Velkovsky).

But whatever the tools, the classical configurations themselves revealed in those analysis happened to be nothing else but our old friend, the *instanton*. Their ensemble saturates the topological susceptibility, solving the U(1) prob-

lem¹. They also do saturate the lowest Dirac eigenmodes, explaining chiral symmetry breaking (again quantitatively, producing accurate value for the quark condensate) and even hadronic correlators, see recent review [1]. I will argue below that instantons explain also the origin of the famous “chiral scale” 1 GeV in QCD [5]. Furthermore, recently instantons emerged as the main driving force in Color Superconductivity.

Instantons also provide few exact results for SUSY theories. They reproduce expansion of the Seiberg-Witten “elliptic curve” for N=2 SUSY QCD [4], and also provide the “master field” of the N=4 theory [6], as discussed here by Mattis.

However many properties of the instanton ensemble are far from being clear. The major example (discussed especially by de Forcrand) is complicated behavior near the critical temperature T_c : qualitative changes in their ensemble are obvious but the structure above T_c is not yet understood.

The only exceptional non-perturbative phenomenon which instantons do *not* explain is confinement [7, 8]: this issue was discussed by Negele.

2 High density QCD

The field of high density QCD was mostly dormant since late-70’s-early 80’s, when implications of perturbative QCD for this case was worked out. However realization last year (simultaneously by “Stony Brook” and “Princeton” groups [17, 18]) that instantons can induced not only strong pairing of quarks with anti-quark in vacuum and break chiral symmetry, but also a quark-quark pairing at high density, has created a splash of activity. Such Color Super-Conducting (CSC) phase was under very intense discussion at the workshop.

It was introduced in the first review talk by F.Wilczek (Princeton), who emphasized the so called color-flavor locking phase [19] which appears for three massless quarks ($N_f = 3$). Discussion of its rather unusual qualitative features was continued by T.Schafer (Princeton), who has presented some quantitative results [20] following from account for instanton interaction. One important result was a demonstration that, as one increase the mass of the strange quark and goes back to the $N_f = 2$ theory, no phase transitions

¹ Not “in principle” (which ’t Hooft did back in 1976), but for real, quantitatively reproduces the value needed to explain correct η' mass.

actually happens and interpolation between two different structures of CSC is in fact continuous. Another interesting issue, for $N_f = 3$ case, is whether there can in principle be a continuous transition from hadronic to CSC phase. Schaefer and Wilczek [22] suggested that the answer is positive.

G.Carter (Copenhagen) had further discussed the $N_f = 2$ case in the instanton model in some details [21], including correct instanton-induced form-factors. R.Rapp (Stony Brook) have provided another view on this subject [20], using statistical rather than mean field description of the instanton ensemble, and discussing the role of instanton-anti-instanton molecules in this transition.

After the workshop an interesting paper written by Son [23] have shown that in the high density (weak coupling) limit (when the instantons are Debye-screened) the leading behavior is not provided by electric (Coulomb) part of the one-gluon exchange, but by a magnetic one.

The talks have so many details that I would not go into it. In summary, QCD demonstrate a kind of “triality”. There are three major phases of QCD: (i) hadronic, dominated by $\bar{q}q$ attraction leading to chiral symmetry breaking; (ii) CSC at high density, dominated by qq attraction and condensation, and (iii) QGP at high T, in which there are no condensates but instantons and anti-instantons themselves are bound by a fermion-induced forces.

A complementary approach to high density QCD, now based on random matrix model, was reviewed by M.Stephanov (Stony Brook). He outlined what exactly goes wrong in “quenched” QCD at finite density, and also how the correct behavior of the Dirac eigenvalue at increasing μ should look like: the resulting picture resembles “a dividing chromosome”, rather than a “cloud” coming from quenched theory. He also pointed out the existence of the tri-critical point at the phase diagram of the random matrix model [24], as well as importance and even possible ways to search for it in heavy ion collisions [25].

Various ideas of how one can proceed to study the high density on the lattice were also discussed. At the end of the talk F.Karsch described new approach, with finite baryon density (instead of chemical potential). M.Alford (MIT) has described possible analytic continuation to complex chemical potential.

Finally M-P.Lombardo (Gran Sasso) had presented very interesting data for 2-color QCD. In this theory the determinant is real even with chemical potential, and so the usual lattice calculations are possible. The results are

consistent with CSC phase being developed.

3 High temperature QCD

Lattice results on finite temperature transitions were reviewed by F.Karsch (Bielefeld) and also by C.DeTar (U. of Utah) . Excellent data for pure gauge theories exist by now, and they show transition at $T_c \approx 260 MeV$. The ratio to the string tension $T_c/\sigma^{1/2}$ is close to $(3/(d-2)\pi)^{1/2}$ as predicted by the string model of deconfinement. M.Wingate (RIKEN/BNL) has presented new data for deconfinement in 4-color gauge theory, which also support this trend.

However, as it is well known by now, QCD with light quarks show *much smaller* critical temperature T_c . This suggests that it has nothing to do with deconfinement, as it is described by the string model.

For 2 light quarks ($N_f = 2$) $T_c \approx 150 MeV$ and is driven by chiral symmetry restoration. The order of the transition in the $N_f = 2$ theory is second, as expected, but “current analysis did not reproduced the expected critical behavior for a system in the universality class of O(4)-symmetric spin models”, Karsch concluded. The situation remains to be quite confusing, the current set of indices do not fit into any of the established universality classes. Maybe the issue is complicated by “approximate restoration of the U(1) symmetry” [32] which add 4 more light (although still massive) modes. If so, the transition may be driven to weak first order instead. DeTar have also shown how lattice artifacts present for $N_t = 4$ and creating doubts about relevance of this case for continuous limit, are actually dissolves for larger values of N_t (up to 12) studied.

DeTar also mentioned interesting simulations by Kogut et al [33] who found weak first order in a simulation in which on top of standard lattice action a small 4-fermion term was added. Let me comment on it: Kogut et al have considered this interaction as a pure methodical tool, they did not specified or speculated about its possible structure. I have however made a point that in fact there is the natural reason why such small interaction should exist: there are small-size ($\rho \sim a$) instantons which “fall through the lattice”. Their contribution should therefore be explicitly added, as another operator into the lattice action.

For the $N_f = 4$ theory, discussed by Mawhinney, the condensate is so

small that the critical temperature is not even measured yet. It however supports a prediction of the instanton liquid model [1] that instanton-induced chiral symmetry breaking should be small at $N_f = 4$ and gone by $N_f = 5$, even at $T = 0$.

The central part of the talk by R.Mawhinney (Columbia) was first results on chiral restoration phase transition using new “domain wall” lattice fermions [34]. The first result is that in this case the chiral symmetry is very accurate², and so one can clearly recognize some zero modes of instantons.

In particular, he discussed also an old question: *what happens in the quenched (pure gauge) theory above T_c ?* Without a determinant, there is no reason for the instantons to be strongly correlated, and if they are more or less random the chiral symmetry should *not* be restored. That contradicted to earlier lattice data, who concluded that chiral symmetry is restored above the deconfinement transition.

One well-understood issue arise here, which may affect recent (not so large-volume) simulations. The total topological charge of the configuration with randomly placed instantons is $Q = |N_+ - N_-| \sim \sqrt{N_+ + N_-}$. Therefore spectrum of the Dirac eigenmodes of quenched configurations should have a term

$$\frac{dN}{d\lambda} = \delta(\lambda) * O(V_4^{1/2})$$

where V_4 is the 4-volume. According to Banks-Casher formula $dN/d\lambda(0) = \pi |\langle \bar{q}q \rangle| / V_4$, but this density does not lead to infinite condensate because it drops out in the thermodynamical limit.

New Columbia data shown by Mawhinney are consistent with this interpretation for $T < T_c$, but *above* T_c the comparison for few volumes available suggested that the coefficient was actually $O(V)$, and the contribution to the condensate therefore is there. He concluded that $\langle \bar{q}q \rangle$ is in fact *infinite* above T_c , not zero as people have claimed before. This is in sharp contrast to earlier works: the measured condensate has changed from 0 to ∞ !

This result can probably be resolved as follows³. At high T the overlap matrix elements between instantons are qualitatively different: instead of decreasing with distance as R^{-3} (as at $T=0$), there appear exponential suppression $\exp(-\pi T r)$ for spatial distance r . Therefore, the whole zone of

²It is broken only by an exponentially small tails of the fermionic wave functions, bound to “plus” and “minus” walls.

³This comment was made in the discussion by T.Schaefer.

instanton-related modes shrinks and it looks as $O(V)\delta(\lambda)$ if the quark mass is not small compared to its width.

True shape of the the zone based on weakly overlapping instantons and anti-instantons⁴ was discussed by Verbaarschot (Stony Brook). His result [36] (recently also confirmed by M.Teper et al[37]) is that in quenched QCD the eigenvalue density actually does grow indefinitely at the origin, but as $dN/d\lambda = O(V)\log\lambda$.

What this means for Columbia results is that for sufficiently small masses (or large length in the 5-th dimension) the singularity in the condensate is going to change from $1/m$ to $\log(m)$. The same behavior should also be there at low T as well, so the quenched theory always has an infinite condensate.

I.Zahed (Stony Brook) has discussed new ideas [35] about “chiral disorder”, connecting motion of light quarks in the QCD vacuum to that of electrons in “dirty metals”. He also proposed two potentially possible regimes for chiral restoration (i) fractal support for the chiral condensate; (ii) either some intermediate phase or specific places on the phase diagram where finite $\langle \bar{q}q \rangle$ (density of eigenvalues) coexist with zero $F_\pi = 0$ (no conductivity) due to eigenmodes localization.

J.Verbaarschot (Stony Brook) have discussed a number of topics about the Dirac eigenvalues. The main point was that zero-momentum sector reduces to Chiral Random Matrix Theory, but it deviates from it at larger eigenvalues [38] He disagreed with Zahed on his last point, arguing (following Parisi) that the localized modes are independent and therefore the fermionic determinant should be a product of the eigenvalues. It strongly mis-favored by any unquenched theory due to smallness of the fermionic determinant, and so he concluded localization scenario is not viable.

M.Engelhardt (Tubingen) have argued that the deconfinement in pure gauge theory can be described de to vortex percolation, rather than monopoles.

4 Lattice instantons at zero and non-zero T

The issue was reviewed by J.Negele (MIT), see [13]. He shown that topological susceptibility is stabilized in many simulations, and the value (dominated

⁴It is better to consider the case when their number is exactly the same, $Q=0$, so that there are no exactly zero topological modes.

by instantons) agrees well with Witten-Veneziano formula. The measurements of the size, defined by extrapolation to the uncooled vacuum, give $\rho = .39 \pm 0.05$ fm. This number, as well as the shape of the size distribution, agrees well with the phenomenology and the instanton liquid calculations. For finite T the size decreases by about 25% by $T = 1.3T_c$, and shrinks at higher T, also in good agreement with the Debye screening mechanism [15, 16].

Negele has shown that most of the smallest fermionic zero modes are related to instantons, both in quenched and full simulations. The important conclusion is that the quark condensate is definitely completely dominated by instantons. Furthermore, restricting the quark propagator to contribution of the lowest modes only, one actually reproduces the correlation functions, not only for such “collective mode” as pions but also for other channels, in particular ρ . Again, this is in agreement which we have found previously by doing correlators in the instanton liquid models.

Another issue Negele discussed based on [7] was the role of instantons in the heavy quark potential and confinement. The conclusion is that the “instanton liquid” does not confine, and contribute to heavy quark potential at the 10-20 % level. The potential found agrees well with other numerical calculations done before, and with analytical one due to Diakonov and Petrov.

There are however three extra points which can be made in connection to this issue. One is that we have found during this investigation that the potential is sensitive to the shape of the Wilson loop, and only if its time dimension T is much larger than spatial one L one gets a correct potential. Diakonov and Petrov recently wrote a rather provocative paper[8], arguing that all existing lattice measurements of the confinement at distances above 1 fm are actually from loops with $L \gg T$, and are therefore suspicious. Unfortunately, simple statistical argument shows that it is practically impossible to go to large enough L in a correct way.

The second point is related with another idea, suggested by Diakonov et al [40], namely that a tail of the distribution at the large-size side may decrease as $dN/d\rho \sim \rho^{-3}$ and lead to infinite confining potential. I think it cannot work, or rather in any way explain what we know about confinement from the lattice. One basic reason is that it would not generate small-size strings, and also generate long-range gluonic correlators. The other is that huge configuration-per-configuration fluctuations of the string tension would

be the case, again contrary to observations.

My third comment is a phenomenological observation, which is by no means new but I think reveal something profoundly important. It is found that quarkonia made of heavy quarks (c,b) and related to confining (and Coulomb) potential have surprisingly small interactions with light quark hadrons. Examples are numerous, let me give one only. Compare two decays with the same quantum numbers of the participants and about the same released energy, $\rho' \rightarrow \rho\pi\pi$ and $\psi' \rightarrow \psi\pi\pi$. The ratio of widths is about a factor 1000! Where this huge factor come from? Only from very different nature of light-quark hadrons (collective excitations of the quark condensate, in a way, as Negele demonstrated) and quarkonia, bound by the confining strings. Why this interaction is *so* small remains unknown.

T.DeGrand and A.Hasenfratz (Boulder) have presented different aspects of their extensive studies of lattice instantons using improved actions [29].

DeGrand reached conclusions similar to Negele's about instantons dominating the smallest eigenvalues, but has shown that instantons alone lead to bad results for the correlators, even the pion one. The difference should be due to different lattice fermions (KS in his work, Wilson in Negele's): in the debate to follow I made a point that in KS case lattice artifacts forbid "collectivisation" of eigenmodes (leading to a scenario similar to what was advocated by Zahed).

A.Hasenfratz (Boulder) described the current status of their work aimed to used "perfect lattice actions" to revealed the true soft content of the quantum configurations. Impressive results for topological observables such as instanton size distribution were presented. The instanton sizes were shown to drift upward, presumably due to mutual attraction, and so the "extrapolation back" seem like a good idea. She had also demonstrated that maybe the best way to "hunt for instantons" is not via very noisy gauge fields, but from lowest fermionic eigenmodes.

One issue discussed in connection to this talks was related to what we actually mean by "total" instanton density. It is clear that as it is done it depends on particular program recognizing instantons. Closed $\bar{I}I$ pairs (or "fluctons" as I have called them in studies of tunneling in quantum mechanics [30]) can only be separated from perturbative fluctuations by some *ad hoc* condition, since there is no real difference between the two. Still, let me point out, to a large extent such pairs can still be well described by semi-classical fields: only instead of the classical fields (minima of the action) we should

look at the “streamline” configurations. Their shapes (and references to the previous works) can be found in [39]: those can well be used for “flucton recognition”.

In summary: the instanton-antiinstanton pairs form the famous valley of $Q=0$ configurations, going smoothly to zero field one. Its population in the vacuum may and can be studied, especially in connection to the long-pending question about understanding of “non-perturbative” aspects of high-order perturbative terms. However, those close pairs do not provide the main object of the instanton physics, the lowest Dirac eigenmodes, and so they would be simply ignored by any fermionic algorithms (like the one discussed by Hasenfratz).

Ph.de Forcrand (Zurich) had also described his version of the “improved cooling” as a way to look for the instantons. He has also observed good agreement between Banks-Casher relation used for the instanton eigenmodes, and the value of the quark condensate. The main topic of his talk however is related with a puzzling question, *what happens at $T > T_c$* for QCD with dynamical quarks?

The proposal by Ilgenfritz and myself [26] was that the ensemble of instantons is broken into so called instanton-anti-instanton molecules. This idea has worked well in the instanton liquid model simulations, see review [1].

However, de Forcrand et al results [27] neither disprove nor completely supported this scenario. On the *pro* side, de Forcrand had demonstrated us that all configuration there have $Q=0$, and that the Dirac eigenvalue spectrum even develops something like a forbidden gap. Many of the smallest eigenmodes do indeed display two maxima in space-time, corresponding to instanton and anti-instanton. There is also some support to our prediction that the molecules should be predominantly oriented in time direction. However, on the *con* side, as seen from de Forcrand’s movie displaying instantons at different T , pure inspection of the action does not provide any clear identification of the $\bar{I}I$ pairs or other clusters in this ensemble. Therefore a change in the spectrum remains a mystery.

In connection to this issue, let me recall recent work by Ilgenfritz and Thurner [28]. Although for quenched configurations only, they have developed a way to correlate relative color orientations of instanton and anti-

instanton. They have measured distribution of the following quantity⁵

$$“\cos\theta” = \frac{\langle G_{\mu\nu}(z_I)UG_{\mu\nu}(z_{\bar{I}})U^+ \rangle}{|G_{\mu\nu}(z_I)||G_{\mu\nu}(z_{\bar{I}})|}$$

where U is transport between centers $z_I, z_{\bar{I}}$. The surprising result is that the distribution is very different at low T and $T > T_c$: the former correspond to random distribution, with $\cos\theta$ peaked around 0, while in the latter case it is peaked at 1 and -1. It probably means, that *even in quenched theory without the determinant* there is some formation of the “molecules”.

Let me summarize the somewhat puzzling situation once again: de Forcrand et al have found only marginal support for the molecular scenario in *full* theory (where it was predicted), while Ilgenfritz and Thurner seem to find them in *quenched* theory (where we did not expected to find them). New simulations, with smaller quark masses (or better, with domain wall fermions) and new way of analysis are needed to clarify it.

5 QCD at larger number of flavors

This is one more direction of the QCD phase diagram, in which we expect chiral symmetry restoration. As it is well known, right below the line at which asymptotic freedom disappears ($N_f = 11 * N_c/2$) the new phase must be a conformal theory because the beta function crosses zero and therefore the theory has an infrared fixed point. We do not however know till what N_f this phase exists, and whether its disappearance and the appearance appearance of the usual hadronic phase (with confinement and chiral symmetry breaking) is actually the same line, or some intermediate phase may also exist in between.

F.Sannino (Yale)⁶ has started this discussion. Based on the gap equation with the one-gluon exchange, Appelquist and collaborators [41] have argued that it should happen close to the line $N_f = 4N_c$, or 12 flavors in SU(3). Another idea suggested by Appelquist et al is the so called “thermodynamical inequality”, according to which the number of massless hadronic degrees of

⁵In fact in order it to be non=zero, it is also necessary to flip sign of the electric component in one of the fields.

⁶He partially presenting his own talk and also substituted T.Appelquist who got ill right before the talk

freedom $N(T = 0)$ can never be larger than the number of fundamental degrees of freedom $N(T = \infty)$. The corresponding numbers at temperature T are defined as

$$N = -F(T) * (90/\pi^2 T^4)$$

If the saturation of it, $N(0) = N(\infty)$, indicates the boundary of hadronic world⁷, one can compare the number of pions $N_\pi = (N_f^2 - 1)$ to the number of gluons and quarks (taken with the coefficient 7/8) and get the same boundary as above.

One may compare these ideas to the boundary found by Seiberg based on his duality considerations and 't Hooft matching anomaly conditions. According to those, the lower boundary of the conformal phase in $N=1$ SUSY QCD⁸ is at $N_f = (3/2)N_c$. The “thermodynamical inequality” of Appelquist remarkably reproduces it!

However (as pointed out by Appelquist et al themselves) the one gluon exchange gap equation actually indicate a *different* point, and, even more important, a completely different pattern of massless particles. The gap equation leads to quark and gluino chiral condensation, but the Seiberg phase has a different set of massless hadrons which are *not* Goldstones, related to chiral symmetry breaking. It probably mean that this approach is too naive. Len me made a suggestion here: one can also get gap equations for the channels favored by Seiberg and see if those can make massless hadrons instead.

As we already mentioned in the section about finite T transition, the instantons can restore chiral symmetry by breaking the random liquid into finite clusters, e.g. $\bar{I}I$ molecules. With increasing N_f this is also happens: it is easy to see if one consider any fermionic line between them as a kind of additional chemical binding bond. At some critical number of those, the entropy of the random phase is no longer able to compensate for binding energy. Explicit simulations suggest it to be at $N_f = 5$, above which the instanton-induced chiral symmetry breaking disappears. This number agrees with a rapid change of the condensate value between $N_f = 3$ and 4 (Mawhinney) and it is also much closer to lattice indications (Iwasaki et al) to the critical

⁷Although I do not understand the reasoning here, sorry. It may somehow be related to 't Hooft matching anomaly conditions, but I was not able to work it out.

⁸Of course, the ordinary and SUSY QCD have different multiplets and beta functions, so we do not mean compare the numbers literally.

point at $N_f = 7$. On the other hand, formation of instanton molecules by no means prevents chiral symmetry breaking by a gluon exchange or any other mechanism (confinement?), and so strictly speaking there is no direct contradiction between two approaches. One may have a strong decrease in a condensate, but not to zero at such $N_f = 5 - 7$.

M.Velkovsky (BNL) discussed a calculation [31] of the vacuum energy density due to such $\bar{I}I$ molecules. He concluded that for $N_f > 6$ there is a difference between even and odd N_f : while for the former the contribution vanishes, for the later it oscillate, changing the sign. It may lead to different (or even alternating) phases at some intermediate N_f .

A very interesting question discussed by Sannino [42](see also [43]) was a question about behavior near the conformal phase boundary. He emphasized that the transition should be infinite order, with not just few but *all* hadronic masses going to zero (see also [43]).

One particular pair of the correlators was discussed by Sannino in particular: those are of two vector and axial correlators. In QCD they are related to rho and a1 excitations, with their parameters approximately related to each other by two Weinberg sum rules⁹ should look like. He has shown that as one becomes close to the transition in question, there appear three separate momenta scales: (i) “partonic” one, $p > \Lambda$, (ii) “hadronic” one $p < |\langle \bar{q}q \rangle|^{1/3}$, and (iii) conformal window in between. The contribution of the part (iii) to Weinberg sum rule, if non-zero, may deform the “hadronic” theory compared to the usual QCD.

V.Elias (U. of Western Ontario) using Pade-summation for beta function, in SUSY and non-SUSy theories E.Gardi (l’Ecole Polytechnique) to penetrate to the boundary of the conformal window, and how far in N_f/N_c can the perturbative theory can actually be used. He concluded that for low N_f such as zero Pade approximant show no indications fro infrared fixed point. He also discussed Kogan-Shifman scenario which appears due to a pole (rather than zero) in the beta function.

E.Gardi also considered the boundary of the conformal window, both in the ordinary and SUSY QCD. He emphasized that bottom of the window correspond to $\gamma = 0$. He concluded in particularly that QCD remains weakly couple in the whole window, which excluded dual description. In SUSY QCD,

⁹Those have zero r.h.s. because QCD have no operators of dimension 2, and also because in the chiral limit the operator of the dimension 4, $G_{\mu\nu}^2$, cancels in the difference.

on the other hand, does become strongly coupled inside the window.

There was a discussion on how exactly people should look for this transition on the lattice. As the transition itself is of “infinite order” because the scale of chiral symmetry breaking is going to the infrared, it should look like rapid decrease of the condensate, with unusual extrapolation to zero. The demonstration of the “conformal window” is much however more straightforward, as it amounts to finding power-like correlators. One more way to see it is to study scaling and construct lattice beta function: it should vanish in the conformal window. In principle, it should converge to the same behavior in the infrared no matter what is the initial charge in the lattice Lagrangian. In reality, the closer it is to the fixed point the better.

6 Some lessons from Supersymmetric Theories

On the onset, let me emphasize one general point. SUSY theories are not a separate class of gauge theories, but rather a particular points on the phase diagrams. One can always enlarge this theories breaking the supersymmetry (e.g. consider the same fundamental fields but different coupling constants). Therefore all features which are not directly caused by SUSY should be true in general. Our general aim is to understand those general dynamical features, to the extent known results in SUSY points can help.

M.Mattis (Los Alamos) had reviewed the status of the instanton calculus for the super-symmetric theories. For $N=2$ SUSY QCD (“Seiberg-Witten theory”) it agrees with expansion of the elliptic curves if $N_f < 2N_c$ but not for the case $N_f = 2N_c$.

Let me inject here a discussion of the amusing similarity between QCD and (its relative) the $N=2$ SUSY QCD have been recently demonstrated in [5]. It is related to the issue of already mentioned “chiral scale” 1 GeV. In QCD it is phenomenologically known that this scale is not only the upper bound of effective theory but also the lower bound on parton model description. However, one cannot really see it from the perturbative logs: 1 GeV is several times larger than their natural scale, $\Lambda_{QCD} \sim 200 MeV$. In the $N=2$ SUSY QCD the answer is known: effective theory at small a (known also as “magnetic” formulation) is separated from perturbative region of large a

by a singularity, at which monopoles become massless and also the effective charge blows up. How it happens also follows from Seiberg-Witten solution, see Fig.2. Basically the perturbative log becomes cancelled by instanton effects, long before the charge blows up due to “Landau pole” at $p \sim \Lambda$. It happens “suddenly” because instanton terms have strong dependence on a : therefore perturbative analysis seems good nearly till this point.

For comparison, in QCD we have calculated effective charge with the instanton correction, as defined by Callan-Dashen-Gross expression. All we did was to put into it the present-day knowledge of the instanton density. The resulting curve is astonishingly similar to the one-instanton one in N=2 SUSY QCD. Note, that in this case as well, the “suddenly appearing” instanton effect blows up the charge, making perturbation theory inapplicable, and producing massless pions, the QCD “magnetic” objects. Moreover, it even happens at about the same place! (Which is probably a coincidence.)

The behavior is shown in Fig.1, where we have included both a curve which shows the full coupling (thick solid line), as well as a curve which illustrates only the one-instanton correction (thick dashed one). Because we will want to compare the running of the coupling in different theories, we have plotted $bg^2/8\pi^2$ (b=4 in this case is the one-loop coefficient of the beta function) and measure all quantities in units of Λ , so that the one-loop charge blows out at 1. The meaning of the scale can therefore be determined by what enters in the logarithm.

The title of Mattis talk is actually ”The Physicist’s proof of the Maldacena conjecture”. In essence, this work [6] is a semi-classical calculation of some specific Green functions in $N = 4$ super-symmetric gauge theory¹⁰, in the large number of colors limit. The multi-instanton “molecules” in this limit becomes dominated by a configuration in which all instantons are at the same place z and have the same size ρ : there is enough space in color space not to worry about their overlap. So, instanton is the “master field” of this approach. The answer obtained is in perfect agreement with Maldacena conjecture and IIB SUGRA calculation, since it looks like classical Green function in which all field propagate from the origination points $x_1...x_n$ to a point in the AdS_5 space, which is nothing but¹¹ $d^4z d\rho/\rho^5$. Additional S_5 also appears, but as a non-trivial space of diquark “condensates” created by

¹⁰E.g. in N=4 theory considered by Mattis all logs are gone and beta function is just zero.

¹¹Let me recall that when I found it, I had a feeling similar to the famous Mollier

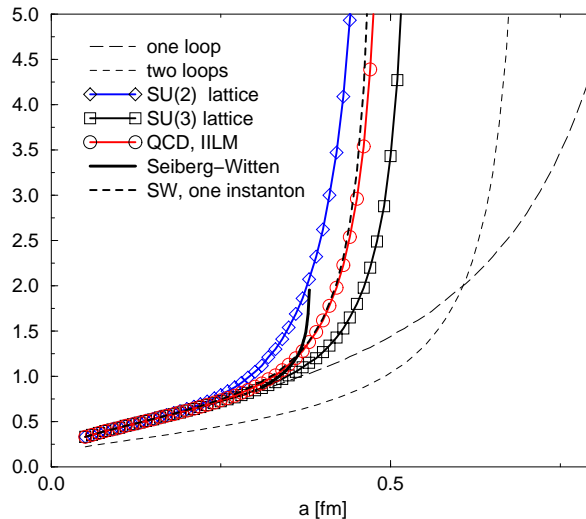


Figure 1: The effective charge $b g_{eff}^2(\mu)/8\pi^2$ (b is the coefficient of the one-loop beta function) versus normalization scale μ (in units of its value at which the one-loop charge blows up). The thick solid line correspond to exact solution [3] for the N=2 SUSY YM, the thick dashed line shows the one-instanton correction. Lines with symbols (as indicated on figure) stand for N=0 QCD-like theories, SU(2) and SU(3) pure gauge ones and QCD itself. Thin long-dashed and short-dashed lines are one and two-loop results.

such molecules.

7 Topological effects in Applications

There were other workshops around (including two October RIVEN workshops and November one in Nordita) dealing with QGP and the phase transition as studied in heavy ion collisions. For that reason we only included in our workshop those talks which have significant overlap with other discussions, such as topology¹² and/or CP violating phases in the θ direction.

A.Zhitnitsky (Vancouver) had literally shocked the audience by his bold character, who just discovered that in all his previous life what he was saying and writing was “prose”.

¹²Not directly related to instantons, which are discussed in other sections.

proposal that the baryon asymmetry of the Universe is *not* due to baryon number violation but rather a large scale *baryon charge separation* in the cosmological QCD transition [44]. He also proposed that all anti-quarks are get locked in the surface of what he calls B-shell, now making the dark matter. The reason it is locked is similar to domain wall fermions: it is a topological bound state resulting from different vacua inside and outside the ball. The sign of the charge is always the same, he explained, because the vacuum inside has a particular CP phase. This meta-stable vacuum related to the (so far rather murky) subject of “other brunches” of QCD vacua as a function of θ parameter.

This development is at its early stage, and it is not possible to tell if it can survive. In a very lovely discussion to follow, several critical comments were made. One of them I made are related to safety issues related to fall on by one of those shells. According to some estimates presented, the baryon charge of the ball is about $B \sim 10^{20}$, or a mass of the order of a gram. If its energy is released in annihilation with matter, it is about an atomic bomb. However Zhitnitsky argued that because the B-shells are large bubbles of another vacuum, the probability of the annihilation should be small.

M.Sadzikowski [45] (Cracow) has demonstrated that earlier estimates of multiple production of baryons and anti-baryons in hadronic and nuclear collisions as a topological defects in chiral models was actually too optimistic. Including realistic quark masses and fluctuations in the same model significantly reduce the rate. His prediction for the rate is about 10^{-4} anti-baryons/fm³.

D.Kharzeev (BNL) addressed the issue of the non-trivial vacuum bubbles with effectively different θ and CP violation [46]. Unlike Zhitnitsky, however, he discussed heavy ion collisions, not cosmology. He argued that high-degree of U(1) restoration may make it possible, although in small vicinity of T_c . The estimates of what the probability of such bubble production are very uncertain. However some ideas how one should look for it were discussed.

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